are illusions of some sort. Their general character* is what would be shown if the adjusting screws of an objective were set up too tightly, producing a set of strains in the glass, or if the objective were strained by its cell. Strains of this sort will sometimes produce faint companions to stars sufficiently bright. A comparison of all the drawings of Venus available in the library of the Lick Observatory is very instructive. All observers, except those at Flagstaff, see faint markings of one class, while those drawn by Mr. Lowell are of a totally different nature.

Venus has been observed on very many occasions at Mt. Hamilton, with our essentially perfect twelve-inch object-glass, in the years 1888–1897, without once seeing markings of the kind drawn by Mr. Lowell, or 'distinct' markings of any kind. Faint and indistinct markings, of the character of those drawn by scores of observers for a century past, are, however, seen when the circumstances are good.

The foregoing notes seem to me to throw doubt on the reality of the markings reported from the Flagstaff Observatory. Until Mr. Lowell's observations are fully comfirmed by other observers with other telescopes, it will be wise not to accept them unreservedly.

EDWARD S. HOLDEN.

MT. HAMILTON, March 9, 1897.

*Six or more radial rays, thicker at the outer rim of the image of the planet.

## FURTHER CONSIDERATIONS ON THE SYSTEMATIC POSITION OF TARSIUS.

PROFESSOR HUBRECHT has replied with some warmth to the paper I lately published in SCIENCE, in which I attempted to show that, in my opinion, *Tarsius* is more of a lemur than an ape, although in reality an annectant type between the two. The objection I raised to placing *Tarsius* among the apes and the effect of this transferral on the classification of the Primates based on their osteology related to recent forms. Professor Hubrecht is probably quite aware that, when we introduce the fossil Primates into the question of classifying the recent forms, the apparently sharp lines of demarcation between the skeletons of recent lemurs and apes disappear.

I entirely agree with Professor Hubrecht in the idea that classification should be based as far as possible on phylogeny, and that the only truly scientific arrangement of animals depends upon a knowledge of their whole organization, both embryonic and adult. I claim, however, that the paleontological method in determining phylogeny is more nearly accurate than the embryological, as in the latter many characters are lost and innumerable cænogenetic variations are introduced which the embryologists often cannot distinguish from real homogenetic structures. The great number of phylogenetic trees based on embryology which are annually cut down is amazing, and in fact the truth of the theory of recapitulation as applied to the embryonic stages is now somewhat questioned.

I do not at all regret quoting the name of Francis Maitland Balfour in regard to his warning against placing too much reliance on placental arrangements as criteria for the classification of the Mammalia, and hold that it applies directly to the question of the systematic position of *Tarsius*. On this side of the Atlantic we do not all follow the Neo-Darwinians in believing that the germinal products are locked up in iron safes as it were, and not affected by external conditions as the rest of the organism.

I will now sum up my principal reasons for not accepting Professor Hubrecht's views that *Tarsius* is only related among the Primates to the Anthropoids:

1. It has not been shown as yet that the placenta in the lemurs is not a derivative of the chorion as in the apes.*

*M. A. Milne-Edwards remarks: "Or, l'allantoide des Indrisines est si facile à detacher des parties adjacentes, qu'il me semble peu probable qu'il ait laissé un de ses feuillets adhérent au chorion, et il y a tout lieu de penser que l'explication mécanique de la production du placenta, telle qu'elle a été proposée par M. Baer et Bischoff n'est pas toujours l'expression de la vérité, et que, dans certain cas au moins, l'arrivée des vaisseaux sanguins de l'allantoïde à la face externe du chorion provoque une hypertrophie dans les parties correspondantes du tissu de cette envellope fœtal, et que c'est de cette manière que se forme le placenta, et non à la suite de l'accolement d'une portion des parois de la vesicule allantoïdienne." Mammiferes de Madagascar, p. 284.

2. The diffuse stage of the placenta of some of the anthropoids is apparently directly comparable to that of the lemurs.

3. There is no fundamental distinction between a large free allantois and one which is rudimentary; it is merely a matter of degree and not one of kind.

4. There is no paleontological evidence as yet deduced which proves that apes and lemurs have arisen independently and that these two phyla were distinct as early as the Mesozoic.

5. The Santa Cruz beds of Patagonia in which *Homunculus* occurs are probably as late as the Lower Miocene.

6. *Anaptomorphus* of the Lower Eocene is much more closely related to the lemurs than to the apes, but it has certain anthropoid characters which indicate that some of the latter may have been derived from this genus.

7. Our present paleontological knowledge indicates that the Old World apes have been derived from a lemurine stock as late as the Oligocene.

8. Synthetic types as *Adapis*, *Tarsius* and *Mesopithecus* demonstrate that apes and lemurs are genetically related.

It remains to be seen whether naturalists in general will be willing to accept Professor Hubrecht's views as to the systematic position of *Tarsius*, depending upon the connection of placenta with the embryo (bauchstiel) and also on the histological details of the former. It seems probable that in forming an opinion as to the affinities of any animal the only judicious course to pursue is to consider the whole organization as well as the development. As far as I can learn from Professor Hubrecht's paper he has not followed this method, but wishes us to accept his conclusions hardly referring to the structures of *Tarsius* which are identical with those of the lemurs and which occur in no other mammalian group except the lemurs.

In conclusion I would like to call Professor Hubrecht's attention to the following passage from Burmeister's Monograph, which shows that he considered *Tarsius* to be a lemur, although Professor Hubrecht does not mention this fact in his memoir: "Aber Tarsius ist nicht mal ein Affe, er ist vielmehr nur ein Halbaffe, ein Mitglied jener Gruppe * * * * *; Darin unterscheidet er sich von allen übrigen Halbaffen und steht eben desshalb so isolirt unter ihnen da." It would be of interest if other morphologists would enter into this discussion and give their opinions as to the systematic position of *Tarsius*. If I did not state Professor Hubrecht's case thoroughly it was an oversight on my part.

Charles Earle.

New Rochelle, New York, April 7, 1897.

## THE COMING ICE AGE.

To the Editor of Science: In Science of March 19th Professor G. Frederick Wright, in his notice of the Coming Ice Age, says that "he is not sure that he has comprehended the author's meaning." And it seems that such is the case where he writes that "the theory of the author is that a land conection between Patagonia and the Antarctic Continent, or a great diminution of the channel between these lands, would produce an effect upon the ocean currents favorable to the glaciation of both hemispheres." This description is so inadequate that it may produce a wrong impression, and so prevent a clear apprehension of what follows in the review. One of the main objects of my explanations has been to show that the closing or diminution of the channel south of Cape Horn would cause the tropical currents to enter the southern seas in sufficient volume to cause an age of mildness in the high southern latitudes which would spread over the globe, and the warm climate would continue until the southern oceans through a slow process retained water sufficient to greatly enlarge the channel south of Cape Horn, and so cause conditions favorable for the glaciation of lands situated in the high southern latitudes, such as is being performed to-day. Consequently, my prognostication of a coming ice age is based on the present enlarged condition of the Cape Horn channel, which affords sufficient space for the strong prevailing westerly winds of that latitude to force the surface waters of the southern oceans through the wide channel and so onward around the globe. Therefore, the